

PEER REVIEW HISTORY

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ARTICLE DETAILS

TITLE (PROVISIONAL)	Authorship inequality: A bibliometric study of the concentration of authorships among a diminishing number of individuals in high-impact medical journals, 2008-2019
AUTHORS	Hart, Kamber; Perlis, Roy

VERSION 1 – REVIEW

REVIEWER	Noel Bairey Merz Cedars-Sinai Medical Center USA
REVIEW RETURNED	19-Nov-2020

GENERAL COMMENTS	<p>1. What is the rationale for including oncology, a subspecialty of internal medicine, and not cardiology which is equally/greater volume? What about other internal medicine subspecialties? What about general surgery and surgical subspecialties?</p> <p>2. Please estimate the impact of many/most journals transitioning from monthly to weekly publications, from paper to online, from including more review, editorial and guideline articles which are more highly cited. Were these variables collected, adjusted for, discussed?</p> <p>3. Please provide a stronger rationale for use of the GC to this non-economic outcome, are there other examples of this measure in non-economic outcomes?</p>
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REVIEWER	Tove Godskesen Centre for Research Ethics & Bioethics, Uppsala University, Uppsala, Sweden, and Department of Health Care Sciences, Ersta Sköndal Bräcke University College, Stockholm, Sweden
REVIEW RETURNED	22-Nov-2020

GENERAL COMMENTS	<p>Thank you for the opportunity to read this interesting paper, close to the field of publication ethics. The authors present the results of a study of authorship inequality to better understand changes in the distribution of authorship among high-impact academic medicine journals over the last 12 years. The paper has the potential to be an important paper and of relevance for the readers of BMJ Open but is unfortunately not ready for publication. I hope the comments below will be helpful.</p> <p>Background: I like the background as it gives the reader a thoughtful picture of what is at stake. At page 6, ICMJE is referred to but no year is mentioned. It would be good to clarify when did they implement stricter guidelines.</p> <p>At page 5: "... the extent to which authorships are concentrated among a small number of senior individuals ... Senior roles are</p>
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	<p>also discussed in the discussion, but what I can see you have not studied whether the authors were senior or junior. So, why this focus? Since this is not part of the aim, I suggest that this theme should be given less focus or be better explained.</p> <p>Methods: Is there any theoretical rationale for choosing medium- to high-impact journals over 12 years? Why high impact journals, and why 12 years? Why 134 journals? Is this part of your hypothesis, that the distribution of authorships should be different in high impact compared to low impact journals? In the aim high impact is used, in other places medium- to high-impact journals. About the method, I am not familiar with Gini Coefficient, but is appeared to be solidly done.</p> <p>Results: The result part is well written, nevertheless hard to grasp and sometimes I had to read the sentence over again to understand the content. For example, it is difficult to understand the passage at page 10-11: "There was a significant positive correlation between year and GC for the overall authorship position ($\rho = 0.99$, $p < 0.001$), the first author position ($\rho = 0.75$, $p = 0.007$), and the last author position ($\rho = 0.85$, $p < 0.001$)". Maybe you can help the reader a bit more to understand the findings.</p> <p>Discussion: The discussion requires being more altered and deepened; the method discussion takes much more space than the discussion of the results. So, my comments here refer to result in the discussion.</p> <p>Several of the references used is books and some places in the discussion. It gives the impression that the authors are not updated in the field, which I assume is wrong. The discussion of the results includes only two references from scientific papers, two books and two self-citations in addition to the one about race/ethnicity. When statements are not problematized, and reference is missing, it might give the impression of being speculative. For example, page 13: "Another potential consequence is academic monoculture - i.e., inhibition of new or less widely-accepted hypotheses that are not supported by a small number of powerful investigators". The only reference here is a book from 2012. After follows an example of the development of treatment for H. Pylori bacterial infection that according to the authors was delayed. Monoculture and delay of developments seem not related to your results or it is said implicitly. Help the reader to understand what you will say. Also, there is no reference to the delay of H. Pylori so the reader cannot check whether this is true.</p> <p>At page 13, pharmaceutical companies are discussed. In what way could authorship's be restricted? And how is this related to your results in this study? The only reference to this passage is a self-citation from the same material.</p> <p>Maybe this article will be of interest for the discussion: Wager E, Singhvi S, Kleinert S. 2015. Too much of a good thing? An observational study of prolific authors. PeerJ 3:e1154 https://doi.org/10.7717/peerj.1154.</p> <p>The concept and problem of hyper-authorship also could be of interest if you find it relevant to discuss (https://www.universityworldnews.com/post.php?story=20191214075947159)</p>
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	Supplementary: There is a reference list with 5 references. I recommend the authors to remove this.
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REVIEWER	Lex Bouter Amsterdam University Medical Centers Department of Epidemiology and Data Science and Vrije Universiteit Amsterdam Faculty of Humanities Department of Philosophy The Netherlands
REVIEW RETURNED	17-Nov-2020

GENERAL COMMENTS	<p>Review of BMJ Open 2020-046002</p> <p>This is a well-written manuscript reporting on trends in authorship inequality in 9 specialties plus interdisciplinary journals in the biomedical literature. Due to innovative and well-described methods it was possible to study more than 300,000 articles published between 2008 and 2019. The main finding that inequality is substantial and increased over time is important in the light of the current criticism on how research and researchers are assessed. By presenting a number of sensitivity analyses and by being open on the study limitations the authors strengthen the credibility of their findings.</p> <p>The manuscript could still be improved by acting upon the following suggestions.</p> <ul style="list-style-type: none"> <input type="checkbox"/> Please try to make the Gini Coefficient more comprehensible by explaining how its numerical value (ranging between 0 and 1) can be interpret. The detailed explanation in the Supplementary methods is clear but I was looking for a one-liner in the main text that more or less correctly states what the numerator and the denominator in this proportion (the GC's numerical value) are. <input type="checkbox"/> Explain whether the study protocol and the data-analysis plan were preregistered. Add the URL to that repository page or explain why no preregistration took place. Also explain why the data set is not being made publicly available in a registry according to the FAIR principles but only made obtainable on request. <input type="checkbox"/> I rather liked the elegant assessment of name homography in the ORCID database. Possibly the connected study limitation that mainly authors with a common name that they share with others will have an ORCID ID is not valid as I believe that currently the large majority of researchers will have an ORCID ID. <input type="checkbox"/> The way authorship positions are allocated and their meaning is indeed probably almost unanimously accepted in the biomedical literature but is radically different in other disciplinary fields. <input type="checkbox"/> Please place your work more clearly in the context of the current debate on criteria for assessing research and researchers and specifically of the pleas to focus less on the number of publications and citations and its derivatives like the Impact Factor and the Hirsch-Index. For this the following sources may provide inspiration. Moher D, Bouter L, Kleinert S, Glasziou P, Sham MH, Barbour V, Coriat AM, Foeger N, Dirnagl U. The Hong Kong principles for assessing researchers: fostering research integrity. PLoS Biology 2020; 18: e3000737
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	<p> https://journals.plos.org/plosbiology/article?id=10.1371/journal.pbio.3000737 https://re.ukri.org/sector-guidance/publications/metric-tide/ http://www.leidenmanifesto.org/ https://sfdora.org/read/ https://rori.figshare.com/articles/report/The_changing_role_of_funders_in_responsible_research_assessment_progress_obstacles_and_the_way_ahead/13227914 </p> <p> <input type="checkbox"/> Maybe place you work in the context of the Matthew effect in Science and refer to: Merton, Robert K. (1968) The Matthew effect in Science, Science, 159 (3810): 56–63. http://fbaum.unc.edu/teaching/articles/Merton_Science_1968.pdf </p> <p> <input type="checkbox"/> Please reflect a bit more on the putative causes of the increase in authorship inequality over time as there may be other drivers next to the perverse incentives connected to authorship and the Matthew Effect. Are junior authors submitting manuscript less often to high impact journals? Are these manuscripts by junior authors rejected more often? Are senior authors misusing the power disbalance by demanding more gift authorships? Etc. </p> <p> <input type="checkbox"/> Maybe discuss to what extent age, career stage, gender, ethnicity and nationality are likely drivers of inequality in authorship and state whether it would be recommendable to investigate this in future research. </p>
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VERSION 1 – AUTHOR RESPONSE

Reviewer: 1

1. What is the rationale for including oncology, a subspecialty of internal medicine, and not cardiology which is equally/greater volume? What about other internal medicine subspecialties? What about general surgery and surgical subspecialties?

We agree, examining other specialties would be valuable for further study, and make our data available to facilitate such work. Here we aimed to present a selection of specialties selected a priori.

2. Please estimate the impact of many/most journals transitioning from monthly to weekly publications, from paper to online, from including more review, editorial and guideline articles which are more highly cited. Were these variables collected, adjusted for, discussed?

We agree with the reviewer that the methods of publication are shifting. While we did not explicitly analyze the transition from monthly to weekly publications or from paper to online publications for individual journals, our sensitivity analysis controlling for the number of articles published per year attempts to assess the impact of these changes.

Additionally, we have clarified the methods section to indicate that these data only analyzed authors from original research articles, excluding editorials, review articles, guidelines, etc. A complete list of article curation is available in our prior work. (10.1001/jamainternmed.2019.0907)

3. Please provide a stronger rationale for use of the GC to this non-economic outcome, are there other examples of this measure in non-economic outcomes?

Thank you for raising this point. The GC has been employed in healthcare research previously including assessment of health-related quality of life and health care resource distributions (Asada, 2005; Jin et al, 2015). We have added these references to the introduction.

Reviewer: 2

Background: I like the background as it gives the reader a thoughtful picture of what is at stake. At page 6, ICMJE is referred to but no year is mentioned. It would be good to clarify when did they implement stricter guidelines.

We have indicated that the stricter ICMJE guidelines on authorship were first updated in 2003.

At page 5: "... the extent to which authorships are concentrated among a small number of senior individuals ... Senior roles are also discussed in the discussion, but what I can see you have not studied whether the authors were senior or junior. So, why this focus? Since this is not part of the aim, I suggest that this theme should be given less focus or be better explained.

We apologize for the confusion. We intended to use our analysis of the last author position as a measurement of the concentration of authorship among a small number of senior individuals. We have broadened this statement to discuss the implications of authorship being concentrated among a few individuals more generally.

Methods: Is there any theoretical rationale for choosing medium- to high-impact journals over 12 years? Why high impact journals, and why 12 years? Why 134 journals? Is this part of your hypothesis, that the distribution of authorships should be different in high impact compared to low impact journals? In the aim high impact is used, in other places medium- to high-impact journals.

Our goal was to analyze articles that were the most relevant to each specialty. For this reason, we scraped articles published in the top 15 journals by impact factor for 9 specialties. Due to the overlap of a few journals between specialties (e.g., *Ultrasound in Obstetrics and Gynecology*, and *Journal of the American Academy of Child and Adolescent Psychiatry*) a total of 134 journals were collected. We have clarified this in the methods section.

While it would be interesting to examine the distribution of authorship among high and low impact journals, this is outside of the scope of this study.

About the method, I am not familiar with Gini Coefficient, but is appeared to be solidly done.

Thank you.

Results: The result part is well written, nevertheless hard to grasp and sometimes I had to read the sentence over again to understand the content. For example, it is difficult to understand the passage at page 10-11: "There was a significant positive correlation between year and GC for the overall authorship position ($\rho = 0.99$, $p < 0.001$), the first author position ($\rho = 0.75$, $p = 0.007$), and the last author position ($\rho = 0.85$, $p < 0.001$)". Maybe you can help the reader a bit more to understand the findings.

We have added expanded the presentation of the results to contextualize the findings in terms of the distribution of authorship, not just high or low GC.

Discussion:

Several of the references used is books and some places in the discussion. It gives the impression that the authors are not updated in the field, which I assume is wrong. For example, page 13: "Another potential consequence is academic monoculture - i.e., inhibition of new or less widely-accepted hypotheses that are not supported by a small number of powerful investigators". The only reference here is a book from 2012. Monoculture and delay of developments seem not related to your results or it is said implicitly. Help the reader to understand what you will say. At page 13, pharmaceutical companies are discussed. In what way could authorship's be restricted? And how is this related to your results in this study? The only reference to this passage is a self-citation from the same material.

We have restructured the discussion to focus on contextualizing these results in prior literature. Therefore, we have removed the section on pharmaceutical companies.

There is no reference to the delay of H. Pylori so the reader cannot check whether this is true.

Thank you for noting this error, we have added the reference.

Maybe this article will be of interest for the discussion: Wager E, Singhvi S, Kleinert S. 2015. Too much of a good thing? An observational study of prolific authors. PeerJ 3:e1154 <https://doi.org/10.7717/peerj.1154>.

The concept and problem of hyper-authorship also could be of interest if you find it relevant to discuss (<https://www.universityworldnews.com/post.php?story=20191214075947159>)

Thank you for these suggestions we have added them to the discussion.

Supplementary: There is a reference list with 5 references. I recommend the authors to remove this.

We have removed the references from the supplemental material.

Reviewer: 3

Please try to make the Gini Coefficient more comprehensible by explaining how its numerical value (ranging between 0 and 1) can be interpreted. The detailed explanation in the Supplementary methods is clear but I was looking for a one-liner in the main text that more or less correctly states what the numerator and the denominator in this proportion (the GC's numerical value) are.

We have added a sentence to the methods and results sections to orient readers to the interpretation of the GC.

Explain whether the study protocol and the data-analysis plan were preregistered. Add the URL to that repository page or explain why no preregistration took place. Also explain why the data set is not being made publicly available in a registry according to the FAIR principles but only made obtainable on request.

Thank you for this suggestion, we will make these data publicly available. The analysis plan was not preregistered as this was a secondary analysis of existing data.

I rather liked the elegant assessment of name homography in the ORCID database. Possibly the connected study limitation that mainly authors with a common name that they share with others will have an ORCID ID is not valid as I believe that currently the large majority of researchers will have an ORCID ID.

We agree with the reviewer that a large majority of researchers have already adopted an ORCID ID. We have tempered the language of this limitation.

♣ The way authorship positions are allocated and their meaning is indeed probably almost unanimously accepted in the biomedical literature but is radically different in other disciplinary fields.

Thank you for recognizing this distinction.

♣ Please place your work more clearly in the context of the current debate on criteria for assessing research and researchers and specifically of the pleas to focus less on the number of publications and citations and its derivatives like the Impact Factor and the Hirsch-Index. For this the following sources may provide inspiration.

Moher D, Bouter L, Kleinert S, Glasziou P, Sham MH, Barbour V, Coriat AM, Foeger N, Dirnagl U. The Hong Kong principles for assessing researchers: fostering research integrity. PLoS Biology 2020; 18: e3000737
<https://journals.plos.org/plosbiology/article?id=10.1371/journal.pbio.3000737>
<https://re.ukri.org/sector-guidance/publications/metric-tide/>

<http://www.leidenmanifesto.org/>
<https://sfdora.org/read/>
https://rori.figshare.com/articles/report/The_changing_role_of_funders_in_responsible_research_assessment_progress_obstacles_and_the_way_ahead/13227914

Thank you for providing these references we have added a section on the implications of using quantitative indicators of academic success to the discussion.

♣ Maybe place you work in the context of the Matthew effect in Science and refer to: Merton, Robert K. (1968) The Matthew effect in Science, Science, 159 (3810): 56–63.
http://fbaum.unc.edu/teaching/articles/Merton_Science_1968.pdf

Thank you for this reference we have added this to the discussion.

- Please reflect a bit more on the putative causes of the increase in authorship inequality over time as there may be other drivers next to the perverse incentives connected to authorship and the Matthew Effect. Are junior authors submitting manuscript less often to high impact journals? Are these manuscripts by junior authors rejected more often? Are senior authors misusing the power disbalance by demanding more gift authorships? Etc.

Thank you for encouraging us to consider upstream factors relating to manuscript submission/acceptance that may influence the distribution of authors. We have added this to the discussion.

Maybe discuss to what extent age, career stage, gender, ethnicity and nationality are likely drivers of inequality in authorship and state whether it would be recommendable to investigate this in future research.

We have added a brief analysis of the role of gender and a comment that additional author-level factors merit further study.

VERSION 2 – REVIEW

REVIEWER	Tove Godsken Centre for Research Ethics & Bioethics, Uppsala University, , Sweden and Department of Health Care Sciences, Ersta Sköndal Bräcke University College, Stockholm, Sweden
REVIEW RETURNED	14-Dec-2020

GENERAL COMMENTS	I find the answer to reviewers well responded and you have taken the suggestions into consideration.
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REVIEWER	Lex Bouter Amsterdam University Medical Centers Department of Epidemiology and Data Science and Vrije universiteit Amsterdam Faculty of Humanities Department of Philosophy The Netherlands
REVIEW RETURNED	09-Dec-2020

GENERAL COMMENTS	The optional suggestions on the initial manuscript were taken into account sufficiently.
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